



FEDERAL SECURITY AGENCY
PUBLIC HEALTH SERVICE

IN REPLYING, ADDRESS THE

Tuberculosis Research Laboratory,
411 East 69th St., New York 21, N. Y.

January 10, 1951.

Dr. Joshua Lederberg,
Department of Genetics,
The University of Wisconsin,
College of Agriculture,
Madison 6, Wisconsin.

Dear Joshua:

*on the other
hand!*

I would be honored at having my *Experientia* paper included in the volume of reprints that you are planning. When the University of Wisconsin Press approached me for an opinion regarding the desirability of such a volume I was not able to express any great enthusiasm, but told them that I don't have enough contacts with students to have a very informed opinion. As to the title, I would think it presumptuous of me to offer any, though I would be very glad to go over any prospectus or outline and offer suggestions at that stage. While I am not familiar with the pros and cons that led you to restrict the topic to bacteria and bacteriophages, it does seem a shame not to include a classic in the *Neurospora* work, such as the original Beadle and Tatum paper (which I haven't read!). I think it very unlikely that I may be familiar with any significant papers in this field that you have missed, but I will get in touch with you if a brainstorm arises. Perhaps it might be worth considering the original short note by Miller and Bohnhoff on streptomycin dependence. I hesitate to plug our own stuff, but since the discovery of POB represents, so far as I know, the first use of mutants to reveal a new growth factor rather than an intermediate, you might be interested in the letter in Nature which should have appeared in the last issue in December 1950, or the first in January 1951.

Sorry my missive seemed so elisive. When we get together you will have no difficulty in seeing how a double mutant was necessary to prove the competition between compound X and shikimic acid; I won't try to fill in what must have been missing from my previous letter. The PABA-POB relationship has already been confirmed with *Rickettsiae* by Snyder at Harvard. I don't think this is likely to tie in with Zalokar's stuff on sulfonamide-requiring *Neurospora*, since our effect requires a high concentration of PABA and clearly represents direct competition, whereas the sulfonamide-requiring *Neurospora* is inhibited by exceedingly little PABA and the inhibition can also be provided by methionine, a product of PABA metabolism.

Dr. Joshua Lederberg

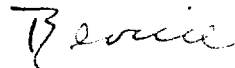
January 10, 1951.

The Plough business makes me very unhappy. He sent a very confused answer, and seems much on the defensive. His intentions are so clearly good that I hate like hell to see it through. I can't agree with you on the best solution being a private one; the really important aspect of the problem seems to me to be that of keeping the literature clean. During a recent visit from Szilard, however, I got persuaded that it would be a mistake to write a letter forcing Plough to publish a retraction. I therefore sent him a short note expressing the hope that he will compare his reversed auxanography with the standard method and will keep me informed. I imagine he will ultimately publish a retraction. If not, I might cover this matter briefly in a review some years hence.

I am glad to have heard from Elise that you will probably have room for her. I think this will work out well. We are planning next fall to have a recent Ph. D. of Elvin Kabat start some serological work since there seems to be room for a good deal of this with the coliforms. We will therefore be eager to keep in touch with the results you get on your new fertile strains.

With regards to Esther, and best wishes for the New Year,

Sincerely,



Bernard D. Davis

BDD/h1

F.S. - We have sent you the W strain under separate cover.